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LOOKING BACK

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Looking back from the end of one's life, one finds its single factors emerge with greater clarity. One of them which has filled my whole scientific life with agony has been writing project proposals. It seems logical that if one asks money from anyone with which to do research, one has to tell what one wants to do with it. However, the situation is not this simple because research means going out into the unknown with the hope of finding something new to bring home. If you know in advance what you are going to do, or even to find there, then it is not research at all: then it is only a kind of honorable occupation. But this is exactly what such proposals are: an account of what one is going to do and expects to find.

Undoubtedly, there are many ways to do research and mine is certainly not the best or only one; but when I go home in the late afternoon from my laboratory I usually do not know what I am going to do the next day. That depends on what I found today, and I need time to digest it, which I mostly do overnight. All the same, all my life I have had to write project proposals, and tell what I was going to do during the next years and why. It has always been an agony to fill up five or ten pages with words. I was not always equally successful in doing so. My last two applications to the American Cancer Society were rejected offhand because I did not tell exactly enough what I planned to do.

My research usually rests on triple basis: I think much, work much, and in doing so I use all my senses. When I have made any new observations they were all due to my noticing some small detail over-

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looked by others. For example: the discovery of ascorbic acid was due to the observation of a small delay in the reaction of peroxidase with benzidine, a reaction done thousands of times daily. It is done even in elementary courses of biochemistry. The discovery of actin and actomyosin rested on the observation that after storage the stickiness of my muscle extracts increased. When I showed this to H. H. Weber, the leader of muscle research, he said that he has seen this many times but thought that his preparation went wrong and sent it down the sink. That I did not do likewise may have been due to the many wild theories I made which prepared my mind. A discovery is said to be an accident meeting a prepared mind. My present research has been stagnant for ten years. I think I have found its solution lately, having noticed an apparently insignificant flatness at one point of my spectroscopic curves. All these observations were unforeseen. I always try to speak the truth, but all my life have had to fill up page after page of my project proposals with untruths. There was no way out. The only alternative would have been to give up research.

I am not the only one who has worked this way. I think it was Claude Bernard who compared research to hunting: one wanders more or less aimlessly till here and there game flies up or one picks up a scent.

Writing papers or books was also agonizing. The light style of some of my writings is misleading. George Wald hit it right when he once said: "This paper of yours is so lightly written that you must have sweated terribly." It was not always so. When I was younger I wrote more easily. Now it is hard work and I cannot decide whether my mind got weaker or my self-criticism stronger, or both. Now I have to rewrite anything I write five times or more. I am not the only one who must do so. The researcher who wrote the clearest papers was O. Warburg. I asked him for his secret. "I rewrite sixteen times," he said. When I write first, I write up everything that comes to my mind. Then I put the paper away and rewrite a month later without looking at my first text. If the second text is different from the first, then I rewrite again. So I may rewrite sixteen times, till the text does not change any more.

In the last five years, I have published least although I worked hardest with the greatest experience and economy; but as I grow older I select more and more difficult problems not only because I have more experience, but also because I think this is *noblesse oblige*. Young people have to establish themselves and so cannot

always risk working on very difficult problems which may not yield results. We elders do not, or should not, depend for our grants and reputation on our daily "publish or perish."¹

The "science explosion" is related to this problem. There is a double explosion: that of knowledge and of the printed word. The explosion of knowledge is not too bad; one can keep up with it. What is bad is the mass of printed words. It is our organization of science that is to blame for it. In giving out grants, we judge, not so much by the depth of the problems or the specific weight of publications, as by the number and weight of papers. This teaches our youngsters not to think in terms of problems, but to think in terms of papers and to choose problems sure to yield a paper.

Science has been caught lately in the crosscurrent of the financial deterioration of this country and its neglect of human priorities. The stipends, and with them the road to science for many young people, have been cut. "Science" is not books but scientists. Depriving our youngsters of the road to science means depriving the future. At the same time, the society is clamoring for the satisfaction of its social needs which can be done only by science. The demand of the public to get something for its tax dollar is legitimate, but there are two ideas which must not be confused. Science is two things. It may be an occupation, like any other—a method for solving problems, whatever they may be. To make a new boot polish is a scientific proposition, but this should not be confused with the work of a Newton, Pasteur, or Gauss, which is high art—in line with Giotto, Rembrandt, or Bach. Both sciences are needed. The first we need to solve our daily problems, some of which threaten our existence; the second we need for a brighter future for man. The weakness of this latter line is that great discoveries always have a lag period; they need time to enrich human life. Practically all of our present pleasures and conveniences—electricity, atomic energy, radio, television, lunar flights—were, for a while, apparently "useless" basic knowledge. So the "great

¹ In my opinion, people who have already shown themselves to be able to do research should not be compelled to write projects at all. They should only be asked to state their needs and the line of their work. Young people who have had no chance yet to show their worth, should rather be judged by elder researchers with or near whom they work. Project proposals are no good measure. The most wonderful proposal in the hands of a poor researcher is useless, while a good man may produce something worthwhile with the poorest proposal or none at all. Young men should be given a chance, but even after having had a chance they should not be judged by their results, but rather by the depth and originality of their thoughts. Deep problems may not yield quick and easy answers. I still remember the discussion of Niels Bohr and Wolfgang Pauli: "You think I am crazy?" Bohr: "I am afraid you are not crazy enough."

science" could be cut out altogether without immediate consequences; but in a decade or two history would become stagnant; industry and even defense would deteriorate.

Progress has but one driving force: human curiosity which, if associated with creativity, produces the great scientists who may seem useless do-nothings, a dispensable social luxury. Newton was one, sitting motionless all day long on Trinity Court. It would have been poor gain to make him work, say, on exhaust gases. All the same, exhaust gases must be dealt with, and only science can do it.

Great science has paradoxical features. The more useful it tried to be, the more useless it would become. If young people ask my advice and tell me they want to go into research because they want to do something good for mankind, I advise them to go rather into charity. Science needs egotists who will satisfy their curiosity at any price. Business thinking is fine, but it fails at creativity. Twice as many plumbers can be expected to do a plumbing job twice as fast, but if one woman can produce a baby in nine months, it does not follow that nine women will be able to produce it in one.

We scientists are accused of many things. Why don't we end war? If all scientists would refuse to collaborate there could be no war. This is correct, but why pick on scientists? If all taxpayers would refuse to pay tax, if all workmen would refuse to work for war, if all students would tear up their draft cards, there could be no war either.

Another reproach often made is that we produce instruments of murder. We do not. What we produce is new knowledge. New knowledge produces new instruments and any instrument can be used both for construction and destruction. Science produces terribly powerful instruments. When they are used for mass murder, the responsibility rests with those who abuse these instruments. It is they who should be eliminated, not science. It is true that we produced the atomic bomb, but we did not do so simply to have it; we had to have it before Hitler did to prevent his world supremacy; we were always against using it to destroy life.

We are also told that we must blame ourselves for our financial plight because we have failed to inform the great public about our work. This is a difficult problem. Scientific research demands utter concentration. If we would spend too much time away from our workbenches we would cease to be scientists and become useless. Publicizing demands a special gift—a flair, which has nothing to do with creativity. Many of our popular writings would probably be terribly

boring. It is for a new breed, the scientific journalist, that this work should be reserved. The problem could be solved also by television, the greatest instrument of communication. If a great channel would reserve time, say half an hour weekly for new scientific discoveries, this would solve the problem and everybody would profit by it.

Many of my young colleagues feel that the time of great discoveries is past. I do not think so. The relation of the known to the unknown has remained practically unchanged. Most problems which demand simple methods may have been solved, but for this we have splendid new instruments to take us deeper into the still endless realms of the unknown, and I regret that my time to use them will be limited.

*TO HELEN LARSON**

Death was the last of God's creations,
And then He rested.
The ameba passed it by, uninterested:
To multiply was to divide
And never die. And never
Be molested.

Those who came in pairs, however,
Found they could not live forever
And gave a sigh in procreation. To multiply
Meant be divested of the flesh;
Only germ cells might slip by
Unto the endless generation
To start afresh.

For God had said, "It is not good
That Death
Should be alone.
I will make a help-meet for him."
And God created sex
Before He rested.
Thenceforth the Law of spawn and die:
To all who might in love
Unite their breath
Came death.

* Helen Larson was the source of the first human cell line to be propagated in vitro.